from time to time observations, of minor importance, perhaps, regarded as isolated facts, but valuable if brought together and studied in relation to those of other observers in the neighbourhood. Not the least service rendered by a volume such as this is that it offers a definite place of record for many a fact which would otherwise probably be lost. We have discussed at some length the biological matters, which occupy about four-fifths of the book. In addition, there is a brief general sketch of the geology, in which, as an instance of interrelation between geology and industry, it may be noted that Woolwich Arsenal is said to owe its establishment to the suitability of the local Thanet sands for iron-moulding. Scientific industries and archæology are other interesting chapters, and there is a concluding note on Woolwich as a centre for photography. The geological section has a very useful bibliography, arranged chronologically. The book is well indexed. W. G. F.

The Flora of the Dutch West Indian Islands. Vol. I., *St. Eustatius, Saba, and St. Martin.* By J. Boldingh. Pp. xii+321. (Leyden: late E. J. Brill, Ltd., 1909.) Price 10*s.*

CONSIDERING the comparative proximity of the West Indies and the number of nationalities in possession, there is a lack of systematic botanical information in the shape of local floras, so that Mr. Boldingh renders good service by the publication of his work relating to three of the Dutch possessions. It is based primarily on his own observations and collections, together with the collections of his countrymen, Dr. Suringar, Mrs. van Grol-Meyer, and Dr. Lionarons, totalling in all about 5000 numbers. The systematic enumeration comprises 806 species, of which 674 are regarded as indigenous and 166 are confined to the West Indies. The Leguminosæ is the best represented family, with sixty species; the Gramineæ, Compositæ, Polypodiaceæ, and Euphorbiaceæ follow in the order named. Panicum, Polypodium, and Peperomia are the larger genera. Ipomœa supplies nine species, of which two are limited to the West Indies, and another is recorded only for St. Eustatius. Two other endemic species, *Galactia nummularia* and *Calyptranthes Boldinghii*, have only been collected on St. Martin.

The author follows Eggers in the ecological divisions, and distinguishes littoral, cultivated, dry shrubby or Croton, and tree or Eriodendron types of vegetation. The dry shrubby and tree vegetations are well developed on St. Eustatius; on Saba the cultivated regions and certain ferns are notable; St. Martin is characterised by the extent of the littoral and shrubby vegetations, while forests are scanty. Generally speaking, the flora of St. Martin differs from that of the other two islands, and contains a number of plants represented on islands lying to the north, while the proportion stands the other way with regard to certain plants recorded only from islands lying to the south. The author has rounded off his information with geological and meteorological notes, a list of vernacular names, chiefly English, and maps. The flora bears out the general view that there is no striking difference between the plants of neighbouring islands in the group.

Weather Forecasting by Simple Methods. By F. S. Granger. Pp. xii+121. (Nottingham: Henry B. Saxton, 1909.) Price 2*s.* 6*d.* net.

THE aim of the author is to provide the means for a single observer "to answer the question 'When will it rain?' in a simple and intelligible manner" without the aid of instruments except a barometer, this, however, being regarded as optional, and not necessary. The methods recommended are based mainly on observations of the size, thickness, extent, height, colours, and forms of clouds. Different aspects of cumulus, cirrus, and stratus cloud are discussed in relation to the weather to be subsequently expected, and isolated examples taken from the author's observations at Nottingham are quoted.

As the result of a long series of observations by an observer who is evidently interested in the subject, the work may prove useful to local observers, but it is doubtful to what extent some of the conclusions arrived at can be considered general. Thus "visibility" is regarded as a sign of good weather, because this phenomenon occurs at Nottingham only during light easterly breezes. In some districts, however, visibility is frequently associated with winds from some westerly point, and is commonly supposed to be a prognostic of rain.

Although Mr. Granger again tells us that meteorology is not an exact but an observational *science*, he says in the same breath that his book is not written on exact and *scientific* lines. He has described it well. His cloud classification is incomplete, and is not that approved by international agreement. He ascribes the formation of cumulus to an electrical cause, and states that lurid red skies in the morning or evening are due to refraction of light. After using the word "gradient" several times he at length defines it as "the slope between two isobars when on one the barometer is one-tenth of an inch higher than the other," and speaks of a gradient of 300 miles, a gradient of 29·9, a shallow gradient, and a form of gradients. There are many other statements which ought to be modified in the light of recent researches. For example, our knowledge of the conditions in the free atmosphere is not as limited as the author suggests, and surely calculation already enters into the science of forecasting, and must continue to do so to an increasing extent.

The arrangement of the work, especially with regard to marshalling the descriptions under some definite plan and arranging them in chapters with appropriate headings, leaves much to be desired. The present arrangement is almost fortuitous.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Invention of the Slide Rule.

I HAVE read with great interest the abstract of the paper on the invention of the slide rule, by Prof. F. Cajori, which appeared in NATURE of December 30, 1909. I agree with the author in thinking that the Rev. William Oughtred was the first to suggest that calculations could be made more accurately and rapidly by sliding the edges of logarithmic scales together than by using compasses —the method adopted by Gunter; but Oughtred had a poor opinion of this device, and rightly considered that his circular scale was a great improvement on it. A few years before 1671, Seth Partridge[1] re-discovered the sliding principle, perfected it, and gave an almost complete specification for the slide rule which is used to-day by engineers.

I was fortunate enough recently to come across, in the library of the British Museum, a pamphlet written by Oughtred in reply to an attack make on him by an instrument maker called Delamain. The pamphlet is entitled "To the English Gentrie and all other studious of the Mathematicks, which shall be readers hereof. The just apologie of Wil: Oughtred against the slanderous insimulations of Richard Delamain, in a pamphlet called Grammelogia, or the Mathematicall Ring." The author very forcibly and very successfully rebuts the charges that were made against him. The following gives his opinion on the question of the priority of the discovery of the circles of proportion:—

1 "The Description and Use of an Instrument called the Double Scale of Proportion." (London, 1671.)

"The honour of the invention, next to the Lord of Merchiston,[1] and our Master Briggs, belongeth (if I have not been wrongly informed) to Master Gunter who exposed their numbers upon a straight line." He then describes the advantages gained by sliding two Gunter's scales together, but points out the defects of this primitive method, and so finally leads up to his circular slide rule.

In the "Epistle Dedicatorie" to Forster's "Circles of Proportion" an answer, said to have been given by Oughtred to a question asking him the reason why he had concealed his inventions so long, is quoted:—

"That the true way of art is not by instruments but by Demonstration; and that it is a preposterous course of vulgar Teachers, to begin with instruments and not with sciences, and so instead of Artists to make their Scholers only doers of tricks, and as it were Juglers; to the despite of Art, losse of precious time, and betraying of willing and industrious wits into ignorance and idlenesse."

Possibly another reason was the fear that his parishioners and others might think that he might have been better employed than inventing slide rules. Supporting this latter view is the fact that he published (1633) his "Mathematicall Recreations" under the pseudonym of Henry Van Etten. In this volume occurs the world-famous arithmetical trick, "Think on a number, double it, &c." It is highly probable that he invented it.

I see no reason for doubting Oughtred's word that he used sliding scales in 1618. The date of Wingate's reputed[2] discovery was thus anticipated by six years. A perusal of Partridge's book published in 1671 shows that the method of using compasses with Gunter's scales was the one that was then generally employed in London. In that year Partridge's slide rules were for sale at the shop of Walter Hayes, at the Cross-Daggers in More-Fields, next door to the Popes-Head-Tavern, London.

Personally, I consider that Seth Partridge is the real inventor of the modern 10-inch slide rule.

ALEXANDER RUSSELL.

Faraday House, London, January 5.

The Tercentenary of the Telescope.

THE article on the tercentenary of the telescope, published in NATURE of December 16, 1909, is extremely welcome, not only because of its appositeness in point of date, but because Dr. Dreyer sets in true light the nature of Galileo's claims in connection with the discovery of the telescope. I do not think that it can be denied that Galileo himself makes the claim, for he puts into the title of the "Sidereus Nuncius" the words "nuper a se reperti." Nor can this be brushed aside as merely an elliptical phrase, because it is pretty clear that he left on the minds of the Doge and Senate of Venice the impression that he had invented the instrument with which he showed them the shipping. I deduce this from the decree as given in a footnote by Mr. Fahie on p. 78 of his admirable "Life of Galileo."

Galileo seems to have known nothing about "the secrets of perspective" as suggested in that decree; he describes quite clearly that he did not reason from optics, but from common sense; and his optics were, in point of fact, wrong when he asserted that one lens could not alone act telescopically. It seems clear that he knew nothing about the formation of an image by a lens. I confess that I cannot see that he is entitled in this matter to so much credit as Prof. Turner ascribes to him in a recent article in the *New Quarterly*.

In the matter of the satellites of Jupiter we tread on much more certain ground, since it is now, I believe, generally conceded that Marius, in his "Mundus Jovialis," gives us a genuine account of his own observations. The charge of plagiarism formulated by Galileo, and repeated by nearly all his biographers, is now exploded. (Mr. Fahie does not explicitly charge Marius with plagiarism, but clearly he disbelieves the general truthfulness of the "Mundus Jovialis," a position that, I feel sure, he would abandon if he read what Messrs. Oudemans and Bosscha have written.) Dr. Dreyer says that Marius found the satellites one day later than Galileo, but when the actual records are compared it becomes clear that Galileo was, on the contrary, at least two days behind Marius. From Galileo's account in his Italian MS. notes, reproduced by Prof. Favaro in his national edition, we see that it was on January 11 that he first suspected the three "stars" to be satellites. (The "Sidereus Nuncius" suggests January 10 for the first suspicions.) Thus Galileo saw them as stars on January 7, and as satellites on January 10 or 11. Now Marius saw them as stars some month or so earlier, and on January 8 he discovered their true nature. Thus it is hardly fair to compare the discovery as satellites made by Marius on January 8 with the mere detection as "stars" made by Galileo on January 7. For the fourth satellite Galileo is entitled to the priority.

I dislike as much as anyone all quarrels about priority, and only direct attention to these facts because of Galileo's hostile attitude. His genius and his intuitive perception of the ways of nature will gain for him for ever the admiration of all men, but his arrogance and jealousy in these two matters make it incumbent on us to be much more critical than in ordinary cases, and particularly so because such fair-minded biographers as Mr. Fahie speak of "his right to the first discovery" of the satellites, and everyone uses the phrase "Galilean telescope."

J. A. HARDCASTLE.

The Dial House, Crowthorne.

[1] Rather a grandiloquent method of referring to Jhone Neper, 'Fear' of Merchiston.

[2] "Le Calcul Simplifié." By M. d'Ocagne. (1905.)

Cross-fertilisation of Sweet-peas.

UNDER the above heading a writer in NATURE of January 6 (p. 280) refers to "the statement that the sweet-pea is invariably self-fertilised," a statement which he thinks is "often based on an opinion of Charles Darwin's." In refutation of this opinion your correspondent describes the visits of the hive-bee and of Megachile to the flower in question. These same species were seen by Mr. Darwin to visit sweet-pea flowers ("Cross and Self-fertilisation," 1876, p. 156). He goes on to ask how it is that the varieties are not habitually mongrelised, and sums up his discussion in the following words:—"Whatever the cause may be, we may conclude that in England the varieties never or very rarely intercross. But it does not follow from this that they would not be crossed by the aid of other and larger insects in their native country, which in botanical works is said to be the south of Europe and the East Indies. Accordingly I wrote to Prof. Delpino, in Florence, and he informs me 'that it is the fixed opinion of gardeners there that the varieties do intercross, and that they cannot be preserved pure unless they are sown separately.'"

January 10. FRANCIS DARWIN.

MAY it be allowable to point out that "π," who has contributed an interesting note (NATURE, January 6, p. 280) on the "Cross-fertilisation of Sweet-peas," is not the same who (vol. lxxii., p. 631) is responsible for the "Rhymes on the Value of π"?

THE ORIGINAL "π."

A Hardy Goldfish.

CAN one of your readers please explain the following incident?

I keep some goldfish in a glass bowl. On December 31 last one of them was seen lying motionless upon its side on the surface of the water. After about an hour, as it was thought to be dead, it was removed to a shelf, remaining there for three hours. My sister then picked it up to throw it away, but was surprised to find it opening its mouth and breathing. She placed it in fresh water, when at first it lay on its side, occasionally moving its head and fins. The water presently appeared to be slightly tinged with the golden colour of the fish, which suddenly turned over on to its back, the ventral surface being upwards, and remained thus for some time. On being transferred to another vessel, the fish, assuming the normal position, swam about leisurely for a little while, and gradually recovered its usual energy, being now equal to any of its old companions.

Was this a case of paralysis, cramp, or other temporary ailment, and what enabled the fish to remain so long alive out of its natural element? G. C. CONSTABLE.

50 Clonmel Road, South Tottenham, January 4.